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Reply on RC1

Ivan Cornut et al.

Author comment on "Potassium limitation of forest productivity – Part 2: CASTANEA-MAESPA-K shows a reduction in photosynthesis rather than a stoichiometric limitation of tissue formation" by Ivan Cornut et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-884-AC1, 2022

General comments

The manuscript submitted to Biogeosciences by Cornut et al. studies the impact of a potassium limitation on wood productivity and in particular on the allocation of carbon and potassium towards trunk, branches and bark, through the modelling and evaluation of the CASTENEA-MAESPA-K model. This is the Part2 of a two-part paper, Part1 being dedicated to the impact of K limitation on the C-source activity (GPP). The split in the 2 parts is relatively well done, and the present manuscript (Part 2) is sufficiently self-supporting, without the need of reading the Part 1 first.

The overall objectives of the manuscript are relevant; and the "Results" and "Discussion" sections report fairly on the model capacities at simulating biomass of each compartment (organ) for different K availability scenarios.

However, the manuscript needs a strong revision of the Methods section and some restructuration. Currently, this section contains too many inconsistencies in several equations, variable units, ... There is not a particular major deficiency but an accumulation of inaccuracies, which really prevents to access at the content of the manuscript. I address many of them in the Technical comments below.

We thank Reviewer one for these general comments. We take notice that our two-part approcah is acknowledged and accepted here. We appreciate that these comments are thorough and relevant to our work. They were very useful when revising our manuscript. We have attempted to answer some of the most important concern below. We hope that our answers can alleviate some of the mis-comprehensions that have stemmed from reading our manuscript. This will feed into the restructuration of the methods section.

Technical comments

I would suggest using the present tense in the Methods section when describing the model features, instead of the past tense.

Line 100-108: As far as I understood, the allocation coefficients presented in subsections 2.3.2 and after are applied over "NPP - C allocated in leaves" not NPP. If this is correct, this should be clarified here. To my understanding, the sentence "The growth of all organs

was a fraction of the daily NPP" does not reflect the way it is modelled. If "all organs" include leaves, it is in contradiction with the sentence "the generation of leaves ... was not directly dependant on NPP". If "all organs" means "all organs except leaves", their growth is a fraction of NPPorg, not NPP. To my opinion, NPPorg should be defined here and not line 200 of the current manuscript.

Thank you for the detailed review, we used a shortcut in our description but yes allocation coefficients are NPP – C allocated to leaves. The production of leaves had priority over the production of all other organs. Meaning that NPP was first allocated to leaves then allocated to the other organs (following their respective allocation ratios). The production of leaves can be limited by NPP in our model but this was very rarely the case in our simulations.

Line 151-152: Could you give slightly more information on how the growth respiration is computed?

Growth respiration was computed by using parameters measured in eucalypt plantations (Ryan et al., 2009). The growth respiration was modelled as in the original CASTANEA model (Dufrêne et al., 2005).

Line 153 and after: Description of the maintenance respiration modelling is quite difficult to follow. This section needs clarifications (see below).

Thank you for this comment. This will be clarified in the manuscript as explained below..

Line 155: maintenance respiration is a "function of their respective respiration rate per nitrogen unit, nitrogen content and surface temperature". Equation (9), in which we divide by N_{trunk}, defines the respiration rate per nitrogen unit (MRN_{trunk}). Equation (10) defines the maintenance respiration from MRN_{trunk} by multiplying it by N_{trunk}. So, RM_{trunk} (or more generally RM_{organ}) does not seeem to be a function of the nitrogen content.

This is true (for the trunk only). We used this since the N content of trunks in Ryan et al., 2009 there were no concurrent measurements of N trunk. Instead we resorted to use a maintenance respiration rate of biomass that was a function of said biomass (to be able to use these published measurements) and a constant Ntrunk for MRN_trunk. This is unclear and will be clarified in the manuscript.

Line 159, equation (9): There is a problem with the 2 terms of the max functions (in brackets). Both terms are constants without any variable.

Yes, there was an error in the transcription of this equation. Thank you for seeing this. The equation should read:

*max(0.00047,0.0073-0.00000116*B_trunk)*1/1e6*3600*1/12*1/0.001*

Line 168: The terms "0.256", "-0.00854" and "0.0759" don't match with the values reported on Figure S2b while those of the equation for $N_{branches}$ do match with values of on Figure S2a.

This will be changed. We will use direct model outputs instead for this figure, that will help clarify the values.

Line 183: "The realised lifespan of leaves was influenced by their K status (see Part 1)". I could not find in Part 1 any information of the equation that relates $LLS_{realised}$ to K status.

This is presented in the companion paper Part 1 (Cornut et al., 2022). Since leaves fall

when their K concentration is below a threshold value, their "true" lifespan can be strongly reduced by K availability.

Line 214 and after, section 2.6.1 Wood: it is the only section where you deal with the cohort level. Is it really needed ? If so, you should better explain the cohorts principle. For instance, line 217-218: "trunk NPP was allocated daily to a cohort of wood". How is this cohort selected among all ?

Wood cohorts correspond to daily productions of wood. Trunk NPP is allocated to the cohort created on that day. This was necessary for our wood remobilisation model.

Line 218-219: "optimal K concentration of newly formed wood was constant and to the maximum trunk concentration measured". In this respect, why equation (15) includes Lim_{org}^{K} which tends to reduce K_{trunk}^{opti}

Because optimal (when K is present in sufficient quantities) and realized (after computation of offer vs demand of K) concentrations are different.

Line 295, equation (20): as it is defined, KUE_{NPP} seems to be a function of the length of the rotation. Is it really expected? It does not appear to be a very handy metric to compare experiments with different rotation length. You could use the mean daily C flux over the length of the rotation instead of the cumulated one. This would imply to express KUE_{NPP} in gC day^{-1} $(gK)^{-1}$

Yes. It is true that this means that KUE is a function of rotation length and that this should not be the focus of this metric. We will thus recalculate its value by using it divided by the number of days in the rotation. This should not change the discussion of the results since we simulated rotations with all the same length.

Line 393: "Potassium concentrations in trunk wood and branches are correctly simulated". Could you provide a quantitative metric for this "correct simulation performance" ? In addition, you should probably highlight that for branches at least, there is a large spread in the measurements in particular for low biomass values.

It is true that this sentence was overly optimistic. We have no hypotheses regarding the large spread of values at low biomasses. It could be an effect due to the large proportion of very young branches. In tne manuscript we will highlight the discrepancies between modelled concentrations in branches and simulated ones.

References:

Cornut, I., Delpierre, N., Laclau, J. P., Guillemot, J., Nouvellon, Y., Campoe, O., ... & le Maire, G. (2022). Potassium-limitation of forest productivity, part 1: A mechanistic model simulating the effects of potassium availability on canopy carbon and water fluxes in tropical eucalypt stands. *EGUsphere*, 1-37.

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Ryan, M. G., Cavaleri, M. A., Almeida, A. C., Penchel, R., Senock, R. S., & Luiz Stape, J. (2009). Wood CO2 efflux and foliar respiration for Eucalyptus in Hawaii and Brazil. *Tree Physiology*, *29*(10), 1213-1222.